



The determinants of cross-border corruption

Laarni Escresa^{1,2} · Lucio Picci³

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Abstract

We reconsider the question of what determines corruption at the cross-national level, using new methods and data: observations of occurrences of cross-national corruption. We find that economic development and a small population is associated with lower levels of corruption, as are freedom of the press, political rights, the presence of established democratic institutions, the salience of women's role in society, and low exports of natural resources such as oil. The particular structure of the data also allows for the first time to consider the “relational aspects” of corrupt relationships, which come to the fore when parties to the corrupt transaction, the briber and the bribee, reside in different countries. Overall, we find limited evidence that the relational factors that we consider affect corruption, beyond the effects that they often have on bilateral trade.

Keywords Corruption · Measures of corruption · Judicial statistics · Culture

JEL Classification H11 · H50 · D73 · C18 · C43 · F53 · F55

1 Introduction

The determinants of corruption have been studied extensively using cross-national data, but reasons can be found that justify addressing the question again. First, the available measures of corruption at the cross-national level have been criticized widely (Charron 2016; Donchev and Ujhelyi 2014; Klitgaard 2017; Knack 2007; Kurtz and Schrank 2007). Second, data on corruption cases at the cross-national level recently have been made available that can serve as an alternative to existing measures (Escresa and Picci 2017). As an added advantage, the new data allow us to consider the relational aspects of corruption,

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✉ Lucio Picci
lucio.picci@unibo.it

¹ Freiburg Institute for Advanced Studies, University of Freiburg, Freiburg im Breisgau, Germany

² School of Economics, University of the Philippines, Quezon City, Philippines

³ Department of Economics, University of Bologna, Strada Maggiore 45, 40125 Bologna, Italy

a topic that has attracted little attention so far. Third, considering occurrences of cross-border corruption is interesting in its own right, given the relevance of the phenomenon.

Research on cross-national corruption most often relies on perception-based measures of corruption, such as Transparency International’s Corruption Perception Index (TI-CPI) (Lambsdorff 1999; Saisana and Saltelli 2012; Transparency International 2012) or the World Bank’s Corruption Control Indicator (WB-CCI) (Kaufmann et al. 2009).¹ As both indexes aggregate differently defined indicators, what they measure exactly is not clear. What is more important, perception-based measures may be only weakly correlated with actual experiences of corruption (Razafindrakoto and Rouband 2010; Seligson 2006; Olken 2009), and psychological mechanisms (Sherif and Cantril 1945) may make perceptions selective so as merely to confirm already existing expectations about country traits. A danger of ‘echo chamber’ effects arises, which is made more acute by the vast media coverage that measures such as the *TI-CPI* have received (Golden and Picci 2005). For instance, Picci (2018) illustrates that availability heuristics appear to have influenced Transparency International’s Bribe Payer’s Index, contributing to a narrative of corruption seen as an element of “national culpability”.²

We consider instead observed cases of cross-national corruption, defined as the bribery by a firm headquartered in a particular country of a public official in a foreign country. The United States, beginning with the Foreign Corrupt Practices Act (FCPA) of 1977, was the first country to criminalize such behavior. On 15 February 1999, the OECD’s Anti-Bribery Convention came into force, requiring signatory countries to adopt similar anti-corruption legislation.³ As a result, many cases of alleged cross-border corruption have been investigated around the world.

The validity of our choice of data and methods rests crucially on the “equal treatment assumption”, according to which the probability that a legal case is filed (in a given jurisdiction), once bribery occurs, does not depend on the identity or characteristics of the country where it took place. For example, if two firms, X and Y, both based in the United States, bribed one public official in Nigeria, and the other in Finland, we assume that the probability that those incidents enter our analysis is roughly the same. We argue that such an assumption is plausible with an appropriate choice of the cases to be considered, and following a series of robustness tests. It must be emphasized that our results obviously do not rest on the assumption that the vigorousness of enforcement of the OECD Convention is even roughly the same around the world—which, as our data also show, is certainly not the case.

We first provide a selective reading of the literature on the causes of corruption, and we then summarize our data, methods and results. In Sect. 6, we also interpret our findings in light of the debate on the “narratives of corruption”, with particular reference to the role played by the available measures of the phenomenon.

¹ See, for reviews, Lambsdorff (2006), Treisman (2007, 2015) and Klitgaard (2017).

² Victimization statistics, obtained from surveys aimed at eliciting information on bribes being paid, have been used more rarely in the relevant studies. On their shortcomings, see Treisman (2007) and Kraay and Murrell (2016).

³ Brewster (2017) provides a convincing explanation of the main drivers of FCPA enforcement. For the determinants of the overall enforcement of the OECD’s Anti-Bribery Convention by signatories other than the United States, see Kaczmarek and Newman (2011) and Choi and Davis (2014). To date, 44 countries have signed the Convention, eight of which are non-OECD countries.

2 The determinants of corruption: a selective survey

Probably the most solidly established result in the available literature demonstrates the negative impact on levels of corruption of economic development, as measured by GDP per capita. The likely presence of endogeneity, together with the difficulty of finding suitable instrumental variables, should be considered when interpreting that and other empirical results (Serra 2006). The availability of abundant natural resources has been associated, albeit not conclusively, with higher levels of corruption (Ades and Di Tella 1999; Serra 2006; Treisman 2007).

A factor that has not received much attention in the literature, but that we consider explicitly, is the size of polities. Mungiu-Pippidi (2015, p. 85) notes that the “size (of population) is not significantly associated with corruption today when all the states of the world are considered, although ... limited population might have played a historical role in enabling collective action”. She points out that most states that have become less corrupt in recent history have small populations. Knack and Azfar (2003) argue that a positive relationship between country size and corruption is evident, but that it likely arises from the presence of a selection bias. Size might affect perceptions of corruption because (everything else being equal) large polities tend to generate more cases of corruption, a possibility consistent with the finding in Escresa and Picci (2017) that more populous countries tend to be significantly less corrupt, according to their measure of corruption based on actual reported corruption cases, the Public Administration Corruption Index (PACI), in comparison with both the TI-CPI and the WB-CCI datasets (see their Table A4).

Levels of corruption are influenced by the extent to which various institutions are able to constrain public officials from rent seeking and opportunistic behavior. In particular, the availability of public information on corrupt exchanges affects the degree to which parties can be held accountable. Evidence shows that countries with greater freedom of the press have lower levels of corruption (Brunetti and Weder 2003; Chowdhury 2004; Treisman 2007). Curtailment of press freedom might take several forms (Freille et al. 2007; Kalenborn and Lessman 2013), such as the level of government spending on newspaper advertisements (Di Tella and Franceschelli 2011).

The effects of formal institutions on corruption have been studied extensively, and the explanatory powers of democratic institutions has been found to be nuanced. Some scholars find a nonlinear relationship between levels of democracy and corruption (Montinola and Jackman 2002; Sung 2004), wherein the relevant conditioning factor is the initial level of democratization, or the level of economic development, as in Charron and Lapuente (2010). Treisman (2007) and Keefer (2007) report that a long history of democracy leads to less corruption.

Other mechanisms by which public officials can be restrained are the vigor of electoral competition and the extent to which different branches of government effectively exercise checks and balances. Persson et al. (2003) find that, in general, lower barriers to political entry, as implied by larger electoral districts and by open electoral lists, are associated with less corruption. A presidential form of government might expand the scopes of rent seeking and corruption, as in Kunicova and Rose-Ackerman (2005), while Gerring and Thacker (2004) report evidence that parliamentary systems, which arguably imply tighter control of the executive branch of government, are less corrupt. Chang and Golden (2010) find that personalistic regimes lead to more corruption compared with military and single-party authoritarian regimes, possibly because the shorter time horizons of personalistic rulers incentivizes the creation of extractive institutions. On the other hand, studies that examine

the relationship between political or fiscal decentralization and corruption have yielded mixed results, both theoretically and empirically (Fisman and Gatti 2002; Treisman 2007; Fan et al. 2009; Fredriksson and Vollebergh 2009; Goel and Nelson 2011).

Cross-country studies point to a negative relationship between the salience of women's roles in society and corruption. Among the explanations offered are the higher standards of ethical and pro-social behavior displayed by women (Dollar et al. 2001), gender-biased socialization mechanisms of the “old boys' club” sort that exclude women from corruption networks (Swamy et al. 2001), or the parallel development of institutions that paved the way both for more gender equality and less corruption (Sung 2003).

The extent to which cultural factors might influence levels of corruption is an object of growing scholarly attention. However, unpacking culture is complicated, and developing quantitative measures to express its different dimensions is problematic. Paldam (2001) finds that religion affects levels of corruption and, in particular, that reform Christianity is beneficial in that respect more so than other pre-reform strands of Christianity. Serra (2006) also confirms that countries that have larger population proportions of Protestants tend to be less corrupt, as does Treisman (2007), who, however, finds that including those factors in the analysis does not change significantly the results for other variables of interest.

Evidence has been reported that the behavior of agents in a foreign country is influenced by the habits and customs of their origin countries. For instance, tax evasion by foreign-owned firms in the United States (DeBacker et al. 2015), and parking violations (Fisman and Miguel 2007) are found to be correlated with corruption levels in the agents' home countries. On the other hand, Picci (2018), using a dataset very similar to the one in this paper, does not find that firms headquartered in more corrupt countries have greater propensities to bribe abroad, once certain control variables aimed at capturing the opportunity to corrupt are taken into account.

Our research also relates to a few works that rely on gravity models to study the relationship between corruption and patterns of trade. Dunlevy (2006) explores how immigrant networks facilitate trade with their countries of origin, possibly because of the advantages they might have in navigating corrupt bureaucracies. Immigrant networks are found to be more useful if the language in the home country is different and institutions are not similar. Dutt and Traca (2010) also use a gravity model to explore whether bribery of customs officials hinders bilateral trade by acting like a tax, or enhances it through the avoidance of tariff barriers. They conclude that in a majority of cases corruption serves as an obstacle to trade, but that in countries with high tariff barriers the marginal observed effect is in fact positive. Using bilateral investment data, Habib and Zurawicki (2002) find that foreign direct investment (FDI) is negatively correlated with levels of corruption in the host country, and positively correlated with the absolute difference in the levels of corruption between the home and the host country. Such studies, however, relate only partially to the present one, which to the best of our knowledge does not have antecedents in explicitly considering, in a cross-country setting, relational variables as determinants of corruption.

3 The data on corruption cases

We rely on an updated version of the dataset used in Escresa and Picci (2017), covering the years from 2000 to 2014. It documents reported cases of cross-border corruption involving firms in a “headquarters country” (indicated herein by the shorthand HQ), and public officials in a “foreign country” (FO). Since a single legal case or enforcement action

lodged against one firm may involve more than one corrupt transaction, we treat each event as a separate case. Cases are coded according to the observed outcome: “positive” if the accused party was either found to be guilty or, while not admitting guilt, agreed to pay a fine (as in a non-prosecution or deferred prosecution arrangement in the United States); “not positive” if the case eventually was dropped or ended in acquittal; or “ongoing” if no available evidence was found to identify it as “positive” or “not positive” (see the Data “Appendix 1” for more details on the dataset).

In the 15-year period covered in our dataset, we observe a total of 1095 cases, irrespective of their outcomes. We recorded information on where enforcement of the case first occurred, either in the headquarters country (627 cases), in a third country jurisdiction (271 cases; the United States was the third country jurisdiction in 172 of them), or in the foreign country (127 cases). For the purpose of our analysis we identify two subsamples of those cases: the larger one comprises the 898 cases that were first enforced either in the headquarters country or in a third country jurisdiction. The smaller subsample includes the 271 cases that were first enforced in a third country jurisdiction.

Table 1 describes the larger of the two subsets, showing the number of cases by headquarters country (top part) and by the foreign public official’s country (bottom part). Of a total of 898 cases, 503 are classified as positive, 288 as ongoing, and the rest as not positive. Firms are headquartered in 42 mostly industrialized countries. First in the list is the United States, reflecting its economic relevance, its early adoption of the FCPA, and the proactive stances taken by the Department of Justice and the Securities and Exchange Commission. The United Kingdom, Germany and France follow in the list. The set of countries in which public officials are at the receiving end of alleged bribes is much broader, with at least one case recorded in a total of 134 countries. China leads that list with 106 cases, followed by Nigeria, Russia and India.

The top panel of the table also shows the headquarters countries that are responsible for about 97.5% of the total number of cases, with the numbers in parentheses indicating cumulative percentages. Just two countries—the United States and the United Kingdom—account for half of the total number of cases. Overall, the dataset includes all countries for which at least one case has been observed in the period under consideration, for a total of 5596 pairwise observations.

Table 2 permits us to better appreciate the rareness of corrupt events in the dataset. Approximately 92% of pairwise observations are zeros—or 96%, when considering cases that were first enforced in third country jurisdictions. Those that are not, most often indicate that only a single case has been observed for a given pair of countries, with higher frequencies occurring sporadically. In the next section, we explain why it is of fundamental importance in our analysis to consider the two subsamples, while excluding the 127 cases that were first enforced in the foreign country.

4 Modeling bilateral corruption transactions

Our estimation strategy observes determinants of corruption of public officials in the foreign country using “observation points” elsewhere (headquarters countries, third country jurisdictions, or both). In order to implement our strategy, we examine only the cases that were first enforced either in the headquarters country or in third country jurisdictions. *We crucially exclude those cases that were enforced first in the foreign country wherein the actual bribery allegedly took place.* By excluding them, we control for the varying

Table 1 Summary of cross-border corruption cases

	Total cases (cumulative % coverage)	Positive cases	Ongoing cases
Number of cases:	898	503	288
<i>By headquarters country</i>			
United States	374 (41.6)	254	82
United Kingdom	76 (50.1)	36	30
Germany	62 (57.0)	29	33
France	54 (63.0)	31	21
Switzerland	46 (68.1)	38	6
Italy	30 (71.5)	6	18
Canada	26 (74.4)	6	11
Spain	22 (76.8)	1	7
Australia	21 (79.2)	9	11
Brazil	19 (81.3)	9	9
Netherlands	17 (83.2)	10	5
Japan	17 (85.1)	12	4
Sweden	17 (87.0)	3	12
Korea	15 (88.6)	15	0
Portugal	12 (90.0)	0	6
China	12 (91.3)	7	4
Argentina, Norway	9 each (93.3)	6	8
Austria, Denmark	8 each (95.1)	7	9
Finland, Israel	5 each (96.2)	10	0
Bermuda, Chile, Hungary	4 each (97.5)	4	3
Others	22 (100%)	9	9
<i>By foreign country</i>			
China	106	66	28
Nigeria	41	30	5
Russia	38	22	11
India	33	19	10
Libya	26	6	17
Indonesia	25	19	5
Brazil	24	12	8
Mexico	22	16	5
Kazakhstan	22	8	9
Angola	20	8	9
Argentina	17	10	7
Thailand	16	14	2
Egypt	16	12	4
Venezuela	16	8	5
Thailand	16	14	2
Others	476	253	163

Cases are those first enforced in the headquarters country or in any third-country jurisdiction. The “headquarters country” is where the firm which allegedly corrupted public officials abroad is headquartered. The “Foreign country” is the country where the act of (alleged) corruption took place. Positive cases refer to cases that were found guilty (see Escresa and Picci 2017 for details). Ongoing cases are those for which we have no information of their conclusion

Table 2 Distribution of cases

	Number of cases observed, with relative frequencies										
	0	1	2	3	4	5	6	7	8	9	≥10
All years	5149	295	85	26	14	9	1	6	0	2	9
	92.01%	5.27%	1.52%	0.46%	0.25%	0.16%	0.02%	0.11%	0	0.04%	0.16%
Cases, regardless of outcome, first enforced not in the FO country (total: 898)	5376	182	30	3	5	0	0	0	0	0	0
	96.07%	3.25%	0.54%	0.05%	0.09%						
Cases, regardless of outcome, first enforced in 3rd country jurisdictions (total: 271)											

The total number of observations is 5596, which equals 42 (the number of headquarters country) times 134 (the number of foreign countries), minus those cases in which the identity of the headquarters and of the foreign country coincides (32 cases). Relative frequencies, in percentages, are not reported when they are equal to zero

propensities of foreign countries to pursue occurrences of corruption involving their own public officials. In different words, it is not the number of cases first enforced in a given jurisdiction, but their *geographic distribution* that is considered to be informative of levels of corruption *outside of that jurisdiction*.

Such a consideration, however, hinges on an “equal treatment assumption”: that a given jurisdiction (which is not the foreign country) acts (as a first enforcer) on a given corrupt transaction involving firms from country i and public officials in country j , with a probability that does not depend on the identity of the foreign country j .⁴

Since the United States is responsible for much of the corruption information available (about 42% of the non-zero observations—see Table 1), a simplistic model for studying the determinants of corruption would focus only on the cases first enforced in the United States involving generic countries j

Simplistic model:

$$cases_{USj} = \beta_0 + \beta_1 X_j^1 + \dots + \beta_k X_j^k + \dots + \delta_1 Z_{USj}^1 + \dots + \delta_q Z_{USj}^q + \epsilon_j$$

where $X_j^1 \dots X_j^k$ represent k characteristics of the foreign country j , whereas $Z_{USj}^1 \dots Z_{USj}^q$ are variables expressing q relational concepts, such as distances and trade flows between the United States and country j .

The obvious shortcoming of such an approach, considering only the United States as the “point of view” of the data, is that it would discard all cases—around 58% of the total—involving firms not headquartered in the United States. To overcome that limitation, cases involving all i headquarters countries ($cases_{ij}$) might be pooled together. The *pooled model*, which is the one that we adopt, also includes country dummies, HQ_i , that control for varying levels of judicial activism.

Pooled model:

$$cases_{ij} = \beta_0 + \beta_1 X_j^1 + \dots + \beta_k X_j^k + \dots + \delta_1 Z_{ij}^1 + \dots + \delta_q Z_{ij}^q + \sum_i \rho_i dummies_HQ_i + \epsilon_{ij}$$

In order to take account of the rareness of corrupt events between two pairs of countries, resulting in many zero observations (see Table 2), we adopt the Poisson estimator, with errors clustered by country pairs. The Poisson estimator has been shown to be appropriate in such cases, notwithstanding the high frequency of zeros (see Silva and Tenreyro 2011), with the advantage of providing results that are invariant to the scale of the dependent variable (unlike, for example, the negative binomial model).⁵ One further advantage of using a Poisson estimator with the present data is that the estimated coefficients can be interpreted

⁴ We refer to the probability that a corrupt transaction is observed *once it has occurred*. On the other hand, the *total number of corrupt transactions* associated with a given foreign country obviously depend on that country’s characteristics. The “equal treatment assumption” corresponds to Assumption 1 in Escresa and Picci (2017, Appendix A), where its role is considered in guaranteeing the validity of their measure of corruption.

⁵ The Poisson estimator is applied frequently in the international trade literature to datasets with structures similar to ours (following Silva and Tenreyro 2006). The known presence of convergence problems led us to use the Windmeijer and Silva (1997) version of the estimator—as implemented in the PPML routine in *Stata*. The occurrence of zeros in the dependent variable might have suggested the use of a zero-inflated formulation of the Poisson model. However, the determination of the presence of corrupt exchanges between two countries, *vis a vis* their intensity, do not seem to be two logically distinct problems, as is somehow implied when estimating such an empirical model. The results might suffer from various forms of endogeneity, which is notoriously difficult to treat because of the dubious validity—or strength—of a rather long list of candidate instruments that have been proposed. See Treisman (2007) for IV results using per-

as elasticities of the impact of the regressors on levels of corruption. On the other hand, values of perception-based indices do not necessarily correspond to known levels of corruption, so that when relying on them, the estimated coefficients are not easily interpreted (see Escresa and Picci 2017).

4.1 The equal treatment assumption

The soundness of our inferential analysis depends crucially on the plausibility of the “equal treatment assumption”. Excluding from the analysis those cases that were first enforced in the foreign country should control for the different levels of judicial activism there. Additional reasons, however, allow us to argue in favor of the plausibility of the equal treatment assumption.

First, note that the assumption is not testable directly, simply because the true number of corrupt transactions is not observable.⁶ Escresa and Picci (2017) provide evidence that the differences between their index of corruption, which is based on a dataset very similar to the present one (and which also excludes those cases that have been enforced first in the foreign country) and the prevailing perception-based measures of corruption, are not driven systematically by the characteristics of the foreign country. That finding might be interpreted as an indicator of equal treatment, conditional on the perception-based measures not suffering from the same bias.

Cases first enforced in the headquarters country likely may emerge (or not) depending on its characteristics—its judiciary and the availability in the headquarters country of relevant information on firms, among others. Information generated in the foreign country occasionally may not result in a local inquiry, but might spur legal action in a different country, which would then act as first enforcer. However, while an accurate analysis in that respect of all the cases considered for the purpose of the present study would represent a daunting task; in the painstaking work that led to the building of our dataset, we did not encounter any such case. It might also be argued that the extent of collaboration between the foreign country’s judiciary and that of the headquarters country might influence the *outcome* of a case. However, collaboration arguably is less important when the focus is on its mere *beginning or discovery*. For that reason, we also present results based on all cases, irrespective of their final dispositions.⁷

We acknowledge that many cases, particularly in the United States, are self-reported by firms. The equal treatment assumption would be violated if firms were more likely to self-report when acting in foreign markets where they perceive a higher risk of being caught, which could depend on the degree of press freedom and on civil liberties in those foreign

Footnote 5 (continued)

ception-based (and also experience-based) measures, and for some comments on the broader issue of finding suitable instruments.

⁶ The determinants of the enforcement of the Convention (or of the FCPA) are not directly relevant and beyond the scope of our study. See for instance, Kaczmarek and Newman (2011) on how FCPA prosecution of non-US corporations might have pushed foreign countries to comply better with the Convention, and Brewster (2017) on how US compliance with the FCPA improved following the adoption of the Convention. Choi and Davis (2014), on the other hand, present an analysis that is *conditional* on FCPA enforcement, focusing on the level of sanctions.

⁷ Several factors influencing the way in which corruption cases are judged, together with the criterion of presumption of innocence, likely lead to many false negatives, thus providing a further justification for considering cases regardless of their outcomes (that is, including acquittals).

countries. However, if those considerations were relevant, we'd expect to find that those variables *positively* influence (detected) levels of corruption, while, as we report below, the opposite is true. We also carry out the analysis excluding all cases first enforced in the United States, where self-reporting arguably is more important, finding results similar to our preferred ones.

The exclusion of all cases first enforced in the United States—both involving firms headquartered there and elsewhere—also addresses two further possible departures from the equal treatment assumption. First, we consider the possibility of so-called industry “sweeps”: the targeting of an entire industry by prosecutors, suspecting the presence of an industrywide pattern of wrongdoing. Inasmuch as such industries interact with foreign countries to different extents, such actions again would imply a departure from the equal treatment assumption. Also, we acknowledge the possibility of “country sweeps”: prosecutors target firms because they are doing business with a particular country, possibly because they believe that that country is characterized by a pattern of wrongdoing (a “culture of corruption”). Arguably, the number of cases is large enough only in the United States for such broad strategies to be of possible relevance. Again, as mentioned above, we also carry out our analysis excluding all cases first enforced in the United States, finding results similar to our preferred ones.⁸

We also acknowledge the possibility that the decision to act as a third country enforcer might be negatively correlated with the foreign country's level of judicial activism. A given jurisdiction might be compelled to initiate an enforcement action involving firms headquartered in another country if it has the impression that corruption would go unchecked otherwise. Excluding all cases first enforced in the United States, which is responsible for most of the third country enforcement, should address that possibility.

The claim that relational characteristics (between the headquarters and the foreign countries) may invalidate the equal treatment assumption would hold even less water, if we limit our attention to cases first enforced in third country jurisdictions (as we also do). For example, if the headquarters country and the foreign country have a long history of reciprocal interactions—possibly because one was a colony of the other (a case that we will control for explicitly)—the probability that an incidence of corruption is detected in the latter might be higher than otherwise. But the same relational characteristic arguably would not affect the probability of detecting a case *in a third-country jurisdiction*.

It should also be noted that the mode of discovery of cases was not just a result of deliberate anti-corruption efforts by law enforcement agencies in the headquarters countries. Some of them emerged in the process of investigating other potential offenses, such as corporate fraud, while others are discovered following the actions of whistleblowers. Also, many of the judicial cases that we consider generate multiple observations, because a given firm allegedly paid bribes in more than one country. The heterogeneity of discovery modes, along with the frequent presence of multiple observations within a single overall corruption case, addresses concerns that cases arise owing to the selective enforcement of governments, either as part of a broader international policy, or as driven by perceptions of corruption.

Last, and notwithstanding all of the previous arguments, a priori knowledge of the likely mechanisms that in principle could invalidate the equal treatment assumption might indicate the direction of the resulting bias. For example, it might be argued that more press

⁸ We cannot rule out the possibility that countries co-ordinate to carry out such industry-, or country-, “sweeps”, but we are not aware of any evidence pointing to the presence such complex form of international coordinated action. We are grateful to Matthew Stephenson for pointing out this and other possible departures from the equal treatment assumption.

freedom in the foreign country could raise (but not lower) the probability that a case surfaces in the foreign country's media and then makes its way to the home country's judiciary, which would act as first enforcer. For that reason, we would have good reasons to be suspicious if the results indicated that more freedom of the press is associated with more corruption. But if the opposite result emerges, as it does, then at most we could suspect that the true effect is even larger than the estimated one.

4.2 Recursive coefficient estimates

We consider cases involving firms headquartered in the 25 countries listed in the top part of Table 1, accounting together for 97.55% of all cases observed. We discard the cases (2.45% of them) originating from firms headquartered in the remaining 17 countries—each of which contributed to fewer than four cases during the 15-year period under consideration. We omit those small countries and the few related cases because when they are considered, their very pronounced infrequency sometimes prevents the Poisson estimator from converging (also see note 5). This must have a very modest effect on our results. To establish that conclusion and to show the overall soundness and appropriateness of the pooling of the “simplistic model” discussed earlier, we estimate recursive coefficients using a simple baseline model. In that model, the number of corruption cases is explained by two regressors only (plus a constant): bilateral logged exports originating in the headquarters country, $\ln(exports)$, which are meant to represent the volume of bilateral transactions between pairs of countries that are vulnerable to corruption, and (logged) per capita income in the foreign country in 1999 ($\ln(gdp\ cap)\ FO$), that is, the year before the beginning of the period covered by the data on occurrences of corruption. We focus on the estimated coefficient of the latter, representing the effect of income on corruption, with the purpose of observing how it changes when we estimate the model many times, progressively adding more “observation points”, i.e., headquarters countries.

Base pooled model:

$$cases_{ij} = \beta_0 + \beta_1 \ln(exports)_j + \beta_2 \ln(gdp\ cap)FO_j + \sum_i \rho_i dummies_HQ_i + \epsilon_{ij}$$

We start by estimating the above model with only the United States as the headquarters country, which alone contributes to 41.6% of the total number of corruption cases (Table 1); note that here, the base pooled model coincides with the previously specified “simplistic model”. We then include the second largest contributor, the United Kingdom, that is, we estimate the pooled model while considering only two headquarters countries, then Germany, and so on, entering one country at a time, eventually including all 42 countries, that is, all observations of occurrences of corruption (first enforced either in the headquarters country or in third country jurisdictions).

We focus on β_2 as our coefficient of interest, representing the impact of logged per capita GDP on levels of the observed occurrences of corruption. In the end, we had 42 estimates of the coefficient of interest, shown in Fig. 1 together with 95% confidence intervals. From left to right, the figure is drawn by gradually including more headquarters countries as “observation points”. The estimated β_2 s always are negative and significant, and they change only modestly as more headquarters countries—and information on cases—are included. In particular, we observe that the estimated coefficient of interest does not change in any appreciable way as we add the last countries, whose firms contribute only a few of the non-zero observations of corruption.

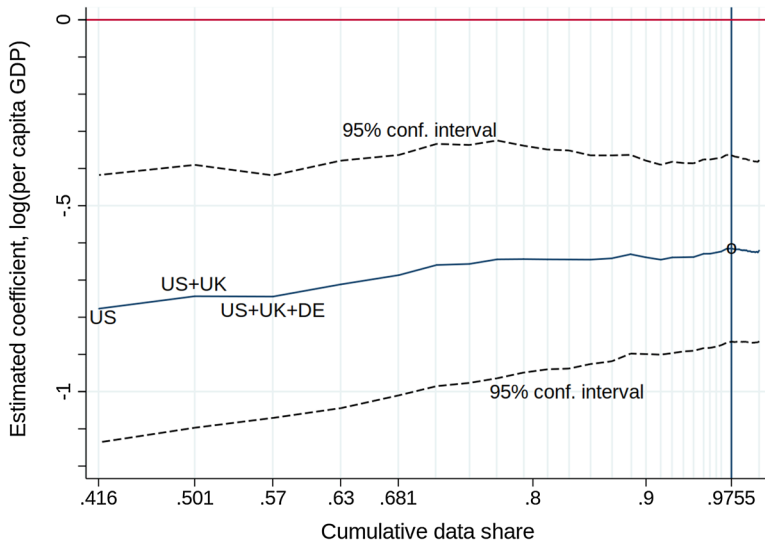


Fig. 1 Estimated impact of GDP per capita, base model. Recursive coefficients. Cases enforced first in the headquarters country and in third country jurisdictions. Note: Point estimates (continuous line) of the coefficient on the per capita income in the base model, together with 95% confidence interval, as additional countries are added. The left-most estimate only includes the US (representing 41.6% of cases), then the US and UK together (representing 50.1% of observations – see Table 1), etc. The thick vertical line represents data coverage (97.55% of total number of observed occurrences) used for main results of paper

The stability of the results, as we move from the single observation point of the simplistic model—the left-most value of the estimated coefficients in Fig. 1—also suggests that the headquarters country dummy variables of the pooled model adequately control for the varying levels of those countries’ judicial activism in prosecuting cases of cross-border corruption.

We carried out the same exercise looking only at cases that were first enforced in third country jurisdictions. Most of those cases (172 out of a total of 271) were adjudicated in the United States and involved firms headquartered elsewhere. Swiss firms represent 15.9% of the cases in that category, followed by France, the United Kingdom and the United States. The recursive coefficients obtained in the exercises are quite stable, as Fig. A1 (in the Appendix) indicates.

5 Estimation results

We now turn our attention to estimates of the pooled model, which are shown in Tables 4 and 5. Table 3 reports, for the reference year 2005, pairwise correlations between the continuous variables that are entered as explanatory factors (for details, see the Data Appendix).⁹ Our choice of variables necessarily is selective, considering the numerous possible

⁹ For several of the variables, we observe large partial correlation, which might lead to multicollinearity. In interpreting the signs of the correlations between variables, attention should be paid to how they are defined (see the Data Appendix). For example, for *Democracy*, higher values correspond to “more”, whereas for *Freedom of the press* the opposite holds.

determinants of corruption identified in the literature. To the extent possible, we follow the choices of Treisman (2007).¹⁰ We also report results for models when the dependent variable includes only the cases first enforced in third country jurisdictions, as shown in Tables A4 and A5 in the “Appendix”, and we refer to them only when they diverge in meaningful ways from those of Tables 4 and 5.

Column 1 of Table 4 contains results for the baseline model, the same for which recursive estimates are shown in Fig. 1. The estimated effect of logged per capita GDP (−0.615) corresponds, in Fig. 1, to the circle on the right-hand vertical line. Logged per capita GDP is significant in most specifications. The log of bilateral exports from the headquarters to the foreign country always are significant, with elasticities that in most estimates are less than one-half, which is markedly smaller than usually is found when estimating gravity models of trade (Disdier and Head 2008). Logged population has a positive effect, which is statistically significant in most specifications, consistent with some of the considerations in Mungiu-Pippidi (2015, p. 85).¹¹ The estimated effect is sizeable, particularly in the results of Table 5, where the estimated elasticity is as large as 34%.

Escresa and Picci (2017) find that populous countries appear to be less corrupt according to the Public Administration Corruption Index (PACI) in comparison with the leading perception-based measures of corruption. Such a finding, on the one hand, is consistent with situations in which perceptions of corruption are *positively* correlated with population size and, on the other, offers indirect support for the authenticity of the positive country-size effects on levels of corruption that we report. The positive effect of logged population also is found in most (but not all) cases when we limit our attention to cases first enforced in third country jurisdictions only—see Tables A4 and A5.

Political rights, freedom of the press, newspaper circulation, and the age of democracy (“Democratic since 1950”) all lead to less corruption. The individual coefficients are statistically significant in most specifications, notwithstanding the extent of collinearity among the variables that emerges from Table 3. Overall, the beneficial effects of proxies for democracy and openness, which are consistent with much of the extant literature (see, among others, Treisman 2007), are one of the clear-cut results emerging from our research.

We find that presidential democracy tends to produce more corruption, as in Kunicova and Rose-Ackerman (2005). In the results of Table 4 (but not of Table A4), we find that open-list electoral systems are associated with more corruption, which is the opposite of what emerges in Persson et al. (2003). We do not find evidence pointing to any effect of district magnitude on corruption, unlike Chang and Golden (2006), nor of pure plurality-rule systems.

Note that when considering the previous four variables, the analysis is conducted on a much smaller subset of countries. The same smaller sample size applies to the next characteristic of governance we consider, namely, a measure of decentralization. In the results of Table 4 (but not of Table A4, which considers only the cases first enforced in third country jurisdictions) we detect a significant positive effect of decentralization. Note, however, that

¹⁰ Treisman (2007) also considers a series of control variables representing historical characteristics of countries, such as their legal origin or colonial past. He finds that entering them does not influence the qualitative results for the other variable of interests. Also, the abundance of fixed effects in our model creates problems in identifying too many time-invariant variables – an issue that is familiar in the international trade literature.

¹¹ Our results do not suffer from the sample bias suggested in Knack and Azfar (2003), since the availability of the dependent variable is not conditional on levels of corruption.

Table 3 Correlation between variables, year 2005

	In (GDP p.c. 1999)	In (GDP p.c. 1999)	Political rights (lower = freer)	FH press freedom	Newsp. circ. 1996	Fiscal decentraliz.	Fuel exports	Imports % GDP	Years opened to trade	Time to open firm
In (GDP p.c. 1999)	-0.1942**									
Political rights (lower = more)	0.0201	-0.3961***								
FH press freedom (higher = freer)	-0.0725	0.3937***	-0.9425***							
Newsp. circ. 1996	-0.0623	0.6868***	-0.5149***	0.5045***						
Fiscal decentraliz.	0.5452***	0.0664	0.0875	-0.1076	0.2507					
Fuel exports	-0.1151	0.1446	0.4964***	-0.4470***	-0.0837	-0.1176				
Imports % GDP	-0.5070***	0.1989**	-0.1653*	0.1577*	0.0739	-0.4451***	-0.1800**			
Years opened to trade	-0.0213	-0.4880***	0.4216***	-0.4489***	-0.4589***	0.1071	0.2142**	-0.2952***		
Time to open firm	0.1462	-0.4051***	0.2575**	-0.3156***	-0.4148***	-0.2146	0.1372	-0.1247	0.4365***	
Women in parliament	0.0400	0.1823*	-0.4185***	0.4353***	0.2517**	0.1225	-0.1888*	0.1623	-0.1966*	-0.3323***
	98	96	99	99	88	41	85	91	86	60

Estimated pairwise correlation (above) and number of observations (below)

***, **, * p value < 0.01, 0.05, < 0.1. For a description of the variables, see "Appendix 1"

Table 4 Pooled model. Dependent variable: all cases enforced first not in the Foreign Country

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Variables</i>						
ln (exports)	0.506*** (0.0736)	0.322*** (0.0615)	0.446*** (0.0558)	0.376*** (0.0672)	0.544*** (0.0692)	0.534*** (0.0645)
ln (population)		0.248*** (0.0571)	0.142** (0.0663)	0.190** (0.0757)	0.124 (0.0965)	0.184* (0.102)
ln (GDP p.c. '99)	-0.615*** (0.128)	-0.331*** (0.113)	-0.0466 (0.0914)	-0.0358 (0.179)	-0.765*** (0.194)	-0.547*** (0.158)
Political rights (lower = freer)			-0.101 (0.0674)	-0.160 (0.110)	-0.248* (0.135)	-0.410*** (0.116)
Democratic since 1950			-0.845*** (0.231)	-0.444* (0.247)	-0.753*** (0.205)	-0.804*** (0.222)
FH press freedom			-0.0202*** (0.00651)	-0.0338*** (0.00802)	-0.0289** (0.0123)	-0.0276*** (0.00975)
Newsp circ. 1996			-0.00345*** (0.000638)	-0.00106 (0.000743)	-0.00148 (0.00113)	-0.00266*** (0.000884)
Presidential dem				0.177** (0.0839)		
Pure plurality syst				0.0831 (0.328)		
Open-list system				0.602*** (0.191)		
District magnitude				0.000539 (0.00187)		
Fiscal decentraliz					0.0127* (0.00666)	
Fuel exports						0.00718*** (0.00252)
Imports % GDP						0.00132 (0.00639)
Yera opened to trade						0.0104 (0.00683)
Time to open firm						-0.311 (0.198)
Women in govt %						-0.0155* (0.00854)
Observations	2985	2985	2616	1325	993	1281
R ²	0.567	0.641	0.508	0.602	0.689	0.718

A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present

entering decentralization results in a loss of significance of the estimated coefficient on logged population. Polity size is correlated rather highly with our measure of decentralization (the correlation coefficient is slightly above 0.5), so prudence is advised when interpreting those two estimated coefficients individually.

Table 5 Pooled model. Dependent variable: all cases enforced first not in the Foreign Country. With relational variables

	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
ln (exports)	0.310*** (0.0849)	0.263*** (0.0676)	0.373*** (0.0657)	0.312*** (0.0634)	0.404*** (0.106)
ln (population)	0.258*** (0.0618)	0.340*** (0.0708)	0.201*** (0.0737)	0.268*** (0.0745)	0.346*** (0.134)
ln (GDP p.c. 1999)	-0.298** (0.131)	-0.232** (0.118)	0.0357 (0.0969)	0.103 (0.103)	-0.297 (0.198)
Political rights (lower = freer)			-0.105 (0.0660)	-0.119* (0.0667)	-0.331*** (0.119)
Democratic since 1950			-0.906*** (0.232)	-0.935*** (0.234)	-1.056*** (0.253)
FH press freedom			-0.0203*** (0.00637)	-0.0216*** (0.00611)	-0.0265*** (0.00966)
Newsp circ. 1996			-0.00325*** (0.000634)	-0.00275*** (0.000656)	-0.00204** (0.000959)
ln (distance)	0.0188 (0.0968)	-0.166 (0.106)	-0.0561 (0.102)	-0.137 (0.106)	0.0495 (0.128)
Contiguous	0.0349 (0.238)	0.0973 (0.232)	0.399 (0.257)	0.418 (0.260)	0.608** (0.308)
Colonial link	0.559* (0.336)	0.549* (0.330)	0.623** (0.253)	0.570** (0.276)	0.826*** (0.317)
Language proximity		-0.488 (0.330)		-0.210 (0.301)	-0.137 (0.487)
Religion proximity		-0.234 (0.224)		0.389* (0.232)	0.832** (0.367)
Religious attitude proximity		0.385 (0.979)		0.658 (0.657)	0.375 (1.269)
Fuel exports					0.00687*** (0.00255)
Imports % GDP					0.00907 (0.00637)
Years opened to trade					0.00662 (0.00685)
Time to open firm					-0.375* (0.208)
Women in govt %					-0.0169** (0.00782)
Observations	2985	2646	2616	2431	1231
R ²	0.612	0.665	0.511	0.510	0.713

A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present

We also enter some variables to capture characteristics of economic governance. We do not find any significant effect of openness to trade, as captured by the share of imports in total product, nor of the variables “Years opened to trade” and of “time to open a firm”. On the other hand, we find that countries exporting more oil tend to be plagued with more corruption, as in Treisman (2007).

Last, countries for which the shares of women among members of parliament are larger tend to be associated with less corruption, confirming results that have been reported in the literature (Alexander et al. forthcoming).

In Table 5, we also consider estimates of models that include relational variables. We omit variables measuring characteristics of democracies, first because we desire to focus on the largest possible set of observations, and also because we found that most of those measures were not significant. Geographic distance is not significant in the results of Table 5, but appears to have a negative effect in some of the specifications shown in Table A5. Geographic contiguity between countries never is found to be significant. In interpreting those results, we should keep in mind that bilateral exports, which are influenced strongly by distance, appear among the regressors. No effect of the distance variable would lead us to conclude that cross-border corruption cases decay with distance faster, or more slowly, than does bilateral trade. A *negative* effect—which we detect in some specifications of Table A5—would imply, on the other hand, that corrupt transactions suffer from geographic distance more than does bilateral trade—or, worded differently, that the “transportation cost” of bribes is higher than that of traded goods.

The presence of a former colonial link is found to have a positive and significant effect on corruption in most specifications. That result should be interpreted in light of what we know about the influence of past colonial links on bilateral trade flows. Head et al. (2010) estimate gravity models to find that past colonial links affect bilateral trade positively, but that such an effect has weakened over time. So, the positive effect of the colonial link on cross-border corruption cases that we detect indicates that past colonial links have disproportionate effects on those cases, that is, even after controlling for bilateral trade. However, once we consider only cases first enforced in third country jurisdictions, while the effect of colonial links on corruption always is estimated to be positive, it never is statistically significant.

We consider additional explanatory variables that are meant to capture cultural proximity between the headquarters and the foreign country. We do not consider the presence of a common language, since we surmise that in the types of corrupt transaction that we observe, potential bribers are (self-)selected so as to be able to communicate their offers in the country where they operate. We also adopt a widely used measure of overall cultural proximity, called *language proximity*, as in Fearon (2003). We likewise consider two different measures of religious attitudes’ proximity. *Religious proximity* is the probability of meeting a person of the same religion, computed on the whole population. *Religious attitude proximity* is the probability that a religious person encounters another religious person, regardless of their particular faiths. We find some evidence that the religious proximity variables might have positive effects on the number of observed cases of corruption, but overall our results indicate little statistical significance for the estimated coefficients on these “cultural” variables.

5.1 Robustness of results

We have seen that restricting our attention to just those corruption cases that were first enforced in third country jurisdictions (Tables A4 and A5), produces results that are very

similar to those that also include cases first enforced in the headquarters country (Tables 4 and 5), even if the two datasets differ significantly (898 vs. 271 observations—see Table 1).

Any departure from the “equal treatment assumption”, on which our results hinge, possibly would affect different countries (taken as “observation points”) differently. The stability of results as we consider different sets of headquarters countries as observation points provides indirect evidence supporting the equal treatment assumption. We commented already on the stability of the recursive coefficients of the baseline model of Figs. 1 and A1.

As a last exercise, we consider the possibility that the United States, as an enforcer of the OECD convention, is an outlier of sorts, considering its early adoption of the FCPA. We estimate all models of Tables 4 and 5 also excluding all cases first enforced in the United States—both involving firms headquartered there and when acting as a third country jurisdiction. With few exceptions, the results (Tables A4-b, A5-b) change only modestly.

6 Discussion and conclusion

In this paper we have presented new evidence on the determinants of corruption and offered two main contributions. First, we proposed a new route for estimating the determinants of corruption at the cross-national level, measured as occurrences of cross-border bribery. By adopting an appropriate estimation strategy, we obtained results that do not suffer from the well-known shortcomings of other measures of corruption at the cross-national level. Moreover, for the first time in a cross-country context, we were able to explore the extent to which relational factors between pairs of countries may facilitate or hinder corrupt transactions.

We find that per capita GDP has a negative effect on corruption. Older democracies tend to be less corrupt, freedom of the press, the salience of women’s roles in society, and the overall extent of political rights are associated with less corruption, while the opposite holds for presidential systems. Of the variables meant to capture characteristics of the economic system of countries, exports of oil favor corruption, a result that can be interpreted as supportive of the so-called natural resource curse. The just-summarized results are not unlike those that have been found in the extant literature.

The concept of corruption that we employ is defined precisely and it is certainly narrower than the vague concept underlying perception-based indicators. A focus on cross-border corruption, like ours, is justified by the relevance of the phenomenon, which often involves important contracts of high value entered into by prominent multinational corporations. However, when applied to corruption at large, our results pose obvious issues of external validity. Public officials may respond differently when dealing with representatives of foreign—versus national—firms. In deciding whether to offer a bribe, representatives of firms doing business abroad might react to the characteristics of the local context differently from local actors. Other evidence, however, indicates that firms with commercial activities in foreign countries tend to mimic their local counterparts. For example, Hellman et al. (2002), using data from the Business Environment and Enterprise Performance Survey conducted by the World Bank and the European Bank for Reconstruction and Development (EBRD) (see <http://data.worldbank.org/data-catalog/BEEPS>), show that foreign firms are as likely as domestic firms to offer kickbacks. Similar results can be found in Gueorguiev and Malesky (2012) and in Soreide (2006).

We believe, however, that comparisons of our results with the extant ones should consider the broader debate on corruption and its determinants. Considering the intrinsic

difficulties in measuring corruption, it is puzzling that perception-based measures have been adopted so widely and nonchalantly, unfortunately also when they definitely should not—as when they are meant to measure changes *in time* (versus across space) of corrupt activities (see the arguments in Escresa and Picci 2016). Experimentation with different measures represents an important research agenda aimed at a better understanding both of their properties, and of the phenomenon of corruption in general.

For the first time, we have presented in a cross-country context an analysis of the effects of relational factors on corruption. We interpreted our results while considering that the same variables also might influence bilateral trade flows, which we include as an explanatory variable. We found scant evidence that the different concepts of country distance that we considered influence corruption “flows” differently from how they might affect bilateral trade, with the exceptions of the two variables representing religious proximity and of past colonial ties, whose significance may have more than one explanation. In terms of their determinants, corrupt cross-border transactions don’t appear to be very different from trade transactions *tout-court*.

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Appendix 1

Data sources, availability and description

Corruption data Version of the dataset used: 3 May 2019. Collection of reported cases of cross-border corruption first used in Escresa and Picci (2017).¹² *Sources:* Trace International Compendium (<http://www.traceinternational.org/compendium>), several US DOJ and SEC documents, OECD (various years), and other databases and publications, such as Shearman and Sterling 2013 (<https://fcpa.shearman.com>), Transparency International 2009 and 2013, Cheung et al. (2012), and Choi and Davis (2014). We cross-checked information also using other news sources, among them the Wall Street Journal Risk and Compliance Journal (<http://www.wsj.com/news/risk-compliance-journal>), and also corruption blogs such as the “FCPA Blog” (<http://www.fcpablog.com>). Cases reported in multiple sources were laboriously consolidated to avoid double counting. The reference period for each case is the year when the bribe was allegedly paid, but in some instances this date had to be presumed from the available data. The term *public official* is used in a broad sense, encompassing both bureaucrats and politicians. Cases where corruption occurs in more than one country are recorded as separate. On the other hand, if more than one bribe is allegedly paid by a firm in a single country within the same occurrence of corruption, only one case is recorded. Cases where the briber is a person (not acting on behalf of a firm) are excluded, as are all the cases pertaining to the Iraq’s “Oil for Food” affair, because of their peculiar characteristics. In the occurrences where more than one jurisdiction took action on a given case, an accurate reading of the available evidence allowed to single out the jurisdiction where the case was first enforced, that is, where it first emerged.

¹² Giana Mildred, Santos Lim and Lorenzo Crippa contributed to different updates of the dataset.

Colonial link Indicates whether two given countries have ever been a colony of the other in modern times. *Source*: Head et al. (2010).

Contiguous A dummy variable indicating the presence of a common border between pairs of countries. *Source*: Mayer and Zignano (2011).

Democratic since 1950 Dummy variable that indicates whether a country has been an electoral democracy since 1950 based on the classification by Beck et al. (2001). *Source*: Treisman (2007).

Distance The distance between the capital cities of any two given countries. *Source*: Mayer and Zignago (2011).

District magnitude Measure of the magnitude of an electoral district using the average number of representatives elected from each electoral district. *Source*: Beck et al. (2001) as cited in Treisman (2007).

Exports Exports between any two given countries. *Source*: United Nations COMTRADE bilateral import/export data, as organized by the Center for International Data (Available at http://cid.econ.ucdavis.edu/Html/WTF_bilateral.html, last accessed on 22 May 2019).

FH press freedom Measure of press freedom based on an evaluation of the legal environment, political and economic factors that contribute towards media independence and access to news and information. *Source*: Freedom House.

Fiscal decentralization Indicators of fiscal decentralization as defined in Fisman and Gatti (2002) which is the share of subnational government spending from total spending of all levels of government. *Source*: Government Finance Statistics, International Monetary Fund as cited in Treisman (2007).

Fuel exports Share of fuel in exports for a given country. *Source*: Treisman (2007).

GDP per capita Year 1999. Measured in current international dollars, PPP *Source*: The World Bank.

Imports % GDP Share of imports out of GDP. *Source*: Treisman (2007).

Language proximity Data from the Ethnologue Project (<http://www.ethnologue.com/>), as collected and organized by James Fearon (2003). The similarity between two languages is based on the distance between “tree branches” (“for example [...] Byelorussian, Russian and Ukrainian share their first three classifications as Indo-European, Slavic, East Branch languages”; Fearon 2003). Unlike in Fearon’s work, who obtains his measure by dividing the number of branches that are in common by the maximum number of branches that any language has (which is equal to 15), we divide it by the maximum number of branches within each couple of language, so as to take into account that the granularity of the branch definition may be not the same across languages”). See also Picci (2010), from which the previous description is taken.

Newspaper circulation The number of newspapers in circulation conditional on the level of democratic liberties for a given country. *Source*: Adsera et al. (2003) as cited in Treisman (2007).

Open list system Indicates whether a country has an open or a closed list system. *Source*: Beck et al. (2001) as cited in Treisman (2007).

Political rights Extent of political rights that exist for a given country or territory. *Source*: Freedom House as cited in Treisman (2007).

Population Population in a given country or territory. *Source*: IMF-World Economic Outlook October 2018.

Presidential dem Treisman’s (2007) measure of presidentialism following Beck’s (2001) classification and where countries with FH scores below 5.5 are assigned a value of 0. *Source*: Treisman (2007).

Pure plurality sys Indicates whether electoral rules in a given country is based on plurality where the most number of votes win (vs. majority rules). *Source:* Beck et al. (2001) as cited in Treisman (2007).

Religious attitude proximity Probability that a religious person in country i encounters another religious person in country j , regardless of their religious membership and affiliation: product of shares of religious persons with respect to the whole population. *Source:* Maoz and Henderson (2013).

Religious proximity Probability of a person in country i meeting another person in country j who belong to the same religion: products of shares of persons with the same religion with respect to the whole population. *Source:* Maoz and Henderson (2013).

SD inflation Measure of variability of inflation based on the annual variance of monthly inflation. *Source:* Braun and di Tella (2004), as cited in Treisman (2007).

Time required to open a firm Time required to complete the regulatory process of a starting a firm. *Source:* Djankov et al. (2002) as cited in Treisman (2007).

Women in government Share of women in parliament (lower legislature). *Source:* Inter Parliamentary Union, as cited in Treisman (2007).

Years opened to trade A variable that indicates the year in which a country opened itself to trade based on Sachs et al. (1995) classification. *Source:* Treisman (2007).

Appendix 2

See Fig. A1 and Tables A4, A5, A4-b and A5-b.

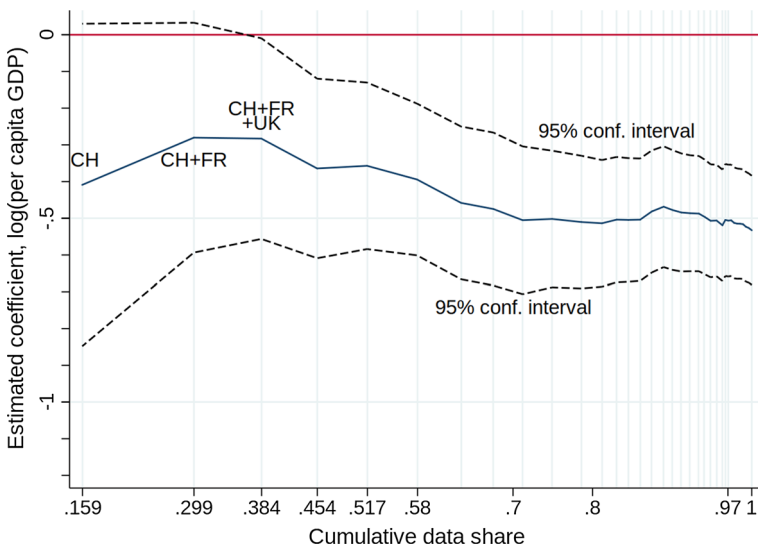


Fig. A1 Estimated impact of GDP per capita, recursive coefficients, base model. Cases enforced first only in third country jurisdictions. Note: Point estimates (continuous line) of the coefficient on the per capita income in the base model, together with 95% confidence interval, as additional countries are added. The left-most estimate only includes Switzerland (representing 15.9% of cases), then the Switzerland and France together (representing 29.9% of observations, etc. Thick vertical line represents data coverage (97% of total number of observed occurrences) used for the results of Tables A4 and A5

Table A4 Pooled model. Dependent variable: all cases enforced first in 3rd country jurisdictions

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Variables</i>						
ln(exports)	0.415*** (0.0399)	0.284*** (0.0645)	0.435*** (0.0742)	0.299*** (0.104)	0.409*** (0.134)	0.586*** (0.0910)
ln(population)		0.191*** (0.0715)	0.129 (0.0926)	0.116 (0.134)	0.117 (0.146)	0.110 (0.160)
ln(GDP p.c. '99)	-0.533*** (0.0758)	-0.338*** (0.108)	-0.0278 (0.114)	0.146 (0.247)	-0.573** (0.259)	-0.433** (0.205)
Political rights (lower = freer)			-0.172* (0.0900)	0.0513 (0.177)	-0.346** (0.175)	-0.321* (0.178)
Democratic since 1950			-1.407*** (0.368)	-0.661* (0.395)	-0.898** (0.395)	-1.099** (0.485)
FH press freedom			-0.0233*** (0.00874)	-0.0172 (0.0115)	-0.0418*** (0.0159)	-0.0268* (0.0145)
Newsp circ. 1996			-0.00451*** (0.00109)	-0.00251** (0.00110)	-0.00277 (0.00171)	-0.00348** (0.00142)
Presidential dem				0.351** (0.148)		
Pure plurality syst				-1.054 (0.655)		
Open-list system				0.266 (0.298)		
District magnitude				-0.00251 (0.00267)		
Fiscal decentraliz					0.00436 (0.0102)	
Fuel exports						0.01000** (0.00422)
Imports % GDP						0.000181 (0.00918)
Years opened to trade						0.0195* (0.0118)
Time to open firm						-0.0632 (0.235)
Women in govt %						-0.00965 (0.00887)
Observations	4139	4139	3108	1316	783	1326
R ²	0.133	0.148	0.143	0.152	0.209	0.238

A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present. Results obtained using all data available

Table A5 Pooled model. Dependent variable: all cases enforced first in 3rd country jurisdictions. With relational variables

	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
ln (exports)	0.232*** (0.0787)	0.298*** (0.0798)	0.307*** (0.0933)	0.376*** (0.0934)	0.324* (0.166)
ln (population)	0.255*** (0.0868)	0.226** (0.0880)	0.262** (0.111)	0.203* (0.113)	0.426* (0.221)
ln (GDP p.c. 1999)	-0.288** (0.117)	-0.349*** (0.124)	0.0897 (0.129)	-0.0286 (0.134)	-0.354 (0.258)
Political rights (lower = freer)			-0.190** (0.0894)	-0.177* (0.0960)	-0.303 (0.184)
Democratic since 1950			-1.460*** (0.386)	-1.343*** (0.397)	-1.089** (0.525)
FH press freedom			-0.0242*** (0.00857)	-0.0242*** (0.00863)	-0.0264* (0.0145)
Newsp circ. 1996			-0.00449*** (0.00115)	-0.00360*** (0.00115)	-0.00301* (0.00158)
ln(distance)	-0.171 (0.118)	-0.195 (0.126)	-0.318** (0.137)	-0.271* (0.147)	-0.211 (0.231)
Contiguous	-0.298 (0.384)	-0.281 (0.386)	-0.0916 (0.369)	-0.184 (0.389)	0.615 (0.493)
Colonial link	0.306 (0.296)	0.114 (0.325)	0.293 (0.318)	0.0981 (0.339)	0.111 (0.437)
Language proximity		0.154 (0.359)		0.253 (0.393)	1.049* (0.592)
Religion proximity		-0.403 (0.299)		0.153 (0.354)	-0.352 (0.640)
Religious attitude proximity		2.114** (0.964)		2.281* (1.318)	0.247 (2.938)
Fuel exports					0.00837* (0.00442)
Imports % GDP					0.00999 (0.0104)
Years opened to trade					0.0204 (0.0130)
Time to open firm					-0.188 (0.243)
Women in govt %					-0.0164* (0.00876)
Observations	4139	3303	3108	2735	1230
R ²	0.146	0.137	0.142	0.146	0.249

A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present. Results obtained using all data available

Table A4-b Pooled model. Dependent variable: all cases enforced non in the foreign country, excluding all cases first enforced in the US

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Variables</i>						
ln (exports)	0.392*** (0.0407)	0.389*** (0.0689)	0.558*** (0.0737)	0.505*** (0.0809)	0.612*** (0.103)	0.624*** (0.0890)
ln (population)		0.00379 (0.0797)	-0.0506 (0.106)	-0.106 (0.113)	0.00322 (0.148)	-0.0409 (0.163)
ln (GDP p.c. '99)	-0.457*** (0.0832)	-0.453*** (0.136)	-0.286** (0.131)	-0.297 (0.225)	-0.882*** (0.270)	-0.800*** (0.249)
Political rights (lower = freer)			0.0486 (0.109)	0.0723 (0.181)	0.0405 (0.201)	-0.355 (0.222)
Democratic since 1950			-1.009** (0.435)	-0.930** (0.422)	-0.899** (0.378)	-1.603*** (0.390)
FH press freedom			-0.00868 (0.0106)	-0.0239* (0.0142)	-0.00448 (0.0183)	-0.0272 (0.0178)
Newsp circ. 1996			-0.00260*** (0.000889)	-3.76e-05 (0.00104)	-0.00115 (0.00134)	-0.00231* (0.00125)
Presidential dem				0.230* (0.130)		
Pure plurality syst				0.134 (0.480)		
Open-list system				0.877*** (0.269)		
District magnitude				-0.000516 (0.00243)		
Fiscal decentraliz					0.0147 (0.0126)	
Fuel exports						0.00469 (0.00456)
Imports % GDP						-0.00745 (0.00877)
Years opened to trade						0.00480 (0.0109)
Time to open firm						-0.687** (0.292)
Women in govt %						-0.000582 (0.00917)
Observations	2766	2766	2422	1220	911	1078
R ²	0.114	0.114	0.149	0.228	0.184	0.161

A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present

Table A5-b Pooled model. Dependent variable: all cases enforced non in the foreign country, excluding all cases first enforced in the US. With relational variables

	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
ln (exports)	0.319*** (0.0857)	0.263*** (0.0846)	0.461*** (0.0909)	0.363*** (0.0958)	0.602*** (0.162)
ln (population)	0.0732 (0.0906)	0.155* (0.0941)	0.0407 (0.107)	0.141 (0.116)	0.0602 (0.192)
ln (GDP p.c. 1999)	-0.325** (0.133)	-0.212 (0.143)	-0.171 (0.132)	-0.0173 (0.149)	-0.614** (0.313)
Political rights (lower = freer)			0.0362 (0.104)	0.0405 (0.104)	-0.231 (0.207)
Democratic since 1950			-1.017** (0.451)	-1.093** (0.449)	-1.725*** (0.377)
FH press freedom			-0.00782 (0.0101)	-0.00976 (0.00964)	-0.0264 (0.0173)
Newsp circ. 1996			-0.00228*** (0.000866)	-0.00203** (0.000898)	-0.00163 (0.00119)
ln (distance)	-0.0809 (0.135)	-0.186 (0.149)	-0.0544 (0.154)	-0.146 (0.161)	0.218 (0.219)
Contiguous	-0.0962 (0.341)	0.00850 (0.362)	0.158 (0.342)	0.218 (0.345)	0.553 (0.460)
Colonial link	1.308*** (0.278)	1.280*** (0.298)	1.102*** (0.277)	1.162*** (0.289)	1.151*** (0.422)
Language proximity		-0.300 (0.404)		-0.278 (0.420)	-0.223 (0.702)
Religion proximity		0.0184 (0.305)		0.742** (0.328)	1.586** (0.627)
Religious attitude proximity		1.298 (0.984)		-0.184 (0.914)	-1.057 (1.910)
Fuel exports					0.00546 (0.00422)
Imports % GDP					0.00560 (0.00891)
Years opened to trade					0.00106 (0.0113)
Time to open firm					-0.696** (0.311)
Women in govt %					-0.00729 (0.00869)
Observations	2766	2536	2422	2330	1078
R ²	0.129	0.126	0.170	0.159	0.189

A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present

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